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Interactive comment on "Comparing a thermo-mechanical Weichselian ice sheet reconstruction to GIA driven reconstructions: aspects of earth response and ice configuration" *by* P. Schmidt et al.

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This is a very comprehensive study comparing leading ice-sheet reconstructions of the Fennoscandian ice sheet between the Last Glacial Maximum and present. As far as I know this is the first study that specifically seeks to compare reconstructions generated via ice-sheet modelling and reconstructions that have been developed within a GIA model. Both the deglacial configurations of the ice sheet and the resulting present day uplift rates due to ongoing postglacial rebound are discussed, and in the latter case model predictions are compared with GPS-derived uplift rates in an attempt to

determine the 'best' ice model. A suite of earth models are also considered in order to determine the viscosity profile that minimises the misfit to present-day uplift rates for each ice-sheet reconstruction. The scientific content of this article is sound, and it constitutes an important contribution to the discussion surrounding the configuration of the former Fennoscandian ice sheet, but there are a number of points that need addressing prior to publication. In particular, the authors would do well to more clearly highlight the novel features and important findings of this work – this does not really come across in the current version.

Main points

1. There is a lot of detailed information in both the abstract and the conclusions, much of which is repeated in the main text. I would suggest re-writing these two sections so that they are more concise – consider the casual reader who may want to quickly read these sections in order to pick out the key findings of the article.

2. In the introduction you only mention one deglaciation model that has been generated using an ice-sheet model (the UMISM model); however, I suspect there are several other groups who have attempted to reconstruct the Fennoscandian ice sheet using ice-sheet modelling techniques. A brief summary of how the UMISM model compares with any other such models would be useful.

3. It may be worth explicitly mentioning within the description of the UMISM model (e.g. page 2350, lines 12-15) that the Earth is modelled differently in the ice-sheet model and the GIA model, leading to a slight inconsistency in the modelling. To what degree might the ice-sheet reconstruction have differed if it had been possible to run UMISM using an earth model similar to that used in the GIA model? E.g. I recall that van den Berg et al. (2008, JGR, doi: 10.1029/2007JB004994) found considerable differences in ice-sheet reconstructions due to feedbacks between isostasy and ice dynamics.

4. Page 2350, line 29: A eustatic sea-level curve that only considers changes to the northern hemisphere ice sheets will underestimate the total eustatic change due to the

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neglect of Antarctic ice volume changes. How do you deal with this issue?

5. There is no such thing as 'observed eustatic sea level'(page 2351, line 21); the location at which eustatic sea level and relative sea level are identical changes over time (see Milne and Mitrovica, QSR, 2008, doi: 10.1016/j.quascirev.2008.08.018), with the result that all sea-level curves should be treated as relative sea-level curves, and these may be used to infer eustatic sea level in combination with GIA modelling. The text should be altered to reflect this.

6. Earth models: As I understand it, the ICE-nG models have been developed in parallel with the VMx Earth models to ensure the best fit to GIA observables such as uplift rates and relative sea-level change – in a couple of places in the manuscript it is suggested that the Earth structure is assumed known prior to the ICE-5G ice-sheet reconstruction, but it is my understanding that checks on the suitability of using the VM2 model will have been carried out throughout the development of ICE-5G, with changes made if necessary. Given the fact that ICE-5G was designed to only be used with VM2, a brief caveat should be included to justify using the ICE-5G ice loading history in conjunction with a range of Earth models that are very different from VM2. I also note that none of the Earth models that you tested approximate VM2 (you run models with a similar viscosity structure, but much thicker values for lithospheric thickness); it would seem sensible to test a model that approximates VM2 before concluding that this model does not fit the data (page 2361, lines 3-6). You also do not explore the full parameter space of the Earth model suggested by Lambeck et al. (2010), in which they determine the lithospheric thickness to lie in the range 65-100km. Given that your best fit (1-layer) earth model for the ANU case lies on the edge of the parameter space that you have explored it would seem sensible to consider (1-layer and 2-layer) earth models with smaller lithospheric thicknesses , to cover the full range suggested by Lambeck et al. (2010).

7. Pre-LGM ice-load changes: In the test described at the top of page 2358 I imagine that there is a discontinuity in ice loading at 36 kyr BP in the hybrid model, when the

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reconstruction switches from the ICE-5G model to the ANU model. How does this affect the results?

8. You do not provide a very strong argument for using the 2007 GPS data, and it would seem more consistent to use the more recent 2010 data in your analysis. Even if you don't adopt the 2010 solution as the main data set, it would be interesting to know whether the chi-squared values are larger or smaller when this solution is used, compared with the 2007 solution.

9. You infer that the over-prediction of uplift rates in Finland by ICE-5G is because the final remnants of the ice sheet retreated to the northern Gulf of Bothnia in this model (page 2363, lines 18-20). However, in figure 7 the misfit seems to be centred over eastern Finland, where there was no ice during the final time steps (e.g. figure 4g). Also, in contrast, there does still seem to be ice over much of northern Sweden towards the end of the deglaciation – a region where the model under-fits the observations. Figure 4 therefore does not seem to support your explanation for the misfit pattern associated with ICE-5G.

10. Given some of the common features of the misfit patterns in figures 7 and 8, have you considered whether lateral variations in earth structure could explain some of the misfit? This issue should probably be borne in mind when suggesting potential edits to the three ice models that you are comparing.

11. Postglacial uplift predictions cannot be directly compared with relative sea-level observations (as advocated on page 2348, lines 26-27) because these are two different physical quantities. The authors attempt to account for this by correcting their uplift predictions using a eustatic sea-level curve. However, sea surface heights in the Ångerman region will differ from eustatic values by >10 m during the early Holocene due to differences in the distribution of surface and internal mass between this time and the present. The error introduced by using eustatic-corrected uplift predictions instead of true RSL predictions will not significantly alter the interpretation of figures 9 and 10,

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but if this comparison is retained in the final version of the manuscript, this issue should be mentioned.

12. Following on from this point, I would question whether the comparison to RSL data is necessary in this paper. You are not comparing the same physical quantities (as discussed above), the comparisons are not quantified, and no real conclusions are drawn aside from the observation that the RSL data can be equally well reproduced using a range of different combinations of ice and earth models. The full Fennoscandian RSL data set provides a very powerful constraint on both the ice and earth components of a GIA model, but since you do not solve the sea-level equation, the usefulness of this data set in this context is limited.

13. English and grammar: Throughout the text there are a number of grammatical errors related to the use of a plural within a sentence (N.B. note that data are plural). 'Ice sheet' does not need a hyphen if it is being used as a noun. Also note that 'the' is missing in a number of cases, in particular when referring to the Younger Dryas or the LGM. Otherwise, the English is generally of a good standard, although the text would benefit from a final read-through to catch any issues that might not be spotted by a non-native English speaker. In the points below I highlight a couple of places where a sentence would benefit from being re-written.

Minor points

Page 2346, line 3: Main > Maine

Page 2346, line 16: later > latter

Page 2347, lines 2-3: It is not clear what you mean by 'the observed ambiguity'

Page 2347, lines 21-22: I was initially surprised that there are no ice thickness data available for the Fennoscandian ice sheet, but then I realised that you are probably referring to the LGM period, when all of the mountains will have been completely covered by ice. It may be worth clarifying this point for the dozy reader.

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Page 2348, line 8: The modelled ice sheet will only behave 'as a real ice sheet' within the bounds of model parameterization of physical processes. I would recommend rephrasing this sentence to reflect the limitations of using models (even good ones) to reflect reality.

Page 2352, line 24: Kara sea > Kara Sea (there are a couple of other places where this correction applies)

Page 2353, line 3: An > A

Page 2353, line 8: ice-lakes > ice-dammed lakes

Page 2353, lines 8-9: Loading due to ice-dammed lakes and marine limit data will be treated very differently within the GIA model so it is not very clear to just say they have both been 'added to the computation' – a little more explanation would be useful for those who are not familiar with how the information is used within the GIA model.

Page 2354, line 3: delete 'of'

Page 2354, line 21: delete 'and'

Page 2355, line 6: glaciation > deglaciation

Page 2357, line6: How has the model been updated since it was benchmarked?

Page 2357, line 9: On the previous page it was stated that a flat earth approximation is used, but here it is stated that the model is extended to a half sphere; how are these statements compatible?

Page 2358, line 24: optional > optimal

Page 2359, equation 1: I think the summation counter should start at i=1

Page 2361, lines 5-7: I would suggest saying that the data are fit well by the model, rather than that the earth model is (or is not) 'well fitted'.

Page 2362, line 11: on > of

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Page 2362, lines 22-23: Saying that the rebound rate will 'become faster' makes it sound like the rate will increase over time. However, what you are trying to say is that the rate in the high viscosity case decreases more slowly than in the low viscosity case, with the result that rebound rates for the low viscosity case drop below the rates for the high viscosity case at some point. There is probably a more succinct way of saying this!

Page 2363, section 5.2: Please make it clear whether you are discussing results for the 1-layer case or the 2-layer case; labelling and referring to the individual plots in figure 7 would be useful. There are also other places where reference to the specific figures being discussed would be useful.

Page 2365, lines 22-24: This sentence is not very clear.

Page 2366, line 4: The use of a double negative makes this sentence rather confusing; this is also an issue at the top of page 2369.

Page 2366, line 8: I suggest revising the phrase 'proper modelling' to something along the lines of 'more realistic modelling'.

Page 2366, lines 17-20: This sentence is muddled.

Page 2367, line 18: to > too

Page 2367, lines 19-21: You suggest that the ANU misfit along the south coast of Norway is very different to the misfit for ICE-5G and UMISM, however, in figure 7 the misfit seems to be very similar for the ANU and ICE-5G models in this region, but it may be that I am misreading the colour scale. On that note, I found it very difficult to distinguish between the colours used in figures 7 and 8.

Page 2369, line 12: to far south > too far south

Figures 9 and 10: Should error bars also be plotted to indicate the age uncertainty of the RSL data?

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